

satellite. As it was in a line continued from the planet through the second satellite, its orbit must necessarily be of a greater dimension than that of the second, I shall therefore put it down as an exterior satellite." Most likely this satellite also was seen amongst the supposed satellites south of the planet, March 27, 1794, where we find mention made "of some others south, at a good distance." In that case this will make a second observation. Will any astronomer be found to call this evidence?

Finally, it is obvious that my subsequent discovery can neither add to nor take from the intrinsic value of Sir William Herschel's observations; and therefore if they do really suffice to reveal the existence of *Umbriel* and *Ariel*, and show their places in the Uranian system, why did not Sir William Herschel, in the long interval between his first and second Paper, logically deduce and prove therefrom their existence? And in his default, why has not some other hand done the work during the half century previous to my discovery? Really the insufficiency of the observations forces itself upon the mind like an axiom—no reasoning can make it plainer.

I remain, dear Sir, yours truly,

WM. LASSELL.

Ray Lodge, Maidenhead,
1874. November 27.

Observations of Iapetus. By W. Lassell, Esq. .

At the request of my friend, Mr. Marth, to whom we are indebted for the published Ephemerides of the Satellites of the planet *Saturn*, I have observed the late superior and inferior conjunctions of *Iapetus*, although the circumstances on both occasions were very unfavourable.

1874, September 24.—At about 20^h 45^m Sidereal Time the planet could be observed for a few minutes only, the sky suddenly clouding before even a diagram could be made. The Satellite was much fainter than I expected. It was situated north of the preceding arm of the ring, perhaps about 40 or 45" from its axis. *Rhea* was near conjunction, south of the same arm, and I noticed that *Iapetus* was much fainter than *Rhea*.

September 25.—At 21^h 8^m Sidereal Time, *Iapetus*, by a mean of several micrometer measures, was 53"·0 north of the major axis of the ring, and exactly in a line with a tangent to the planet's following limb, perpendicular to that axis. The air was muddy and disturbed.

November 2.—The sky was perfectly cloudy. I need

scarcely remark that, as the true conjunctions both happened in the daytime, all that could be done was to observe if possible on the preceding and following evenings in both instances.

November 3.—At about 21^h, Sidereal Time, the sky partially cleared in a very hazy manner, showing *Saturn* only for a very short time, and scarcely at all without thin clouds. A diagram, however, was somewhat hastily made of the planet and its surrounding satellites, *Rhea* and *Dione* being to the east, and *Tethys* to the west, considerably short of its greatest elongation. I estimated *Iapetus* to be about 50" south of the major axis of the ring, in a perpendicular raised from the middle of the preceding arm.

The power used was 579, on the two-foot Equatoreal. I accounted *Iapetus* to be slightly brighter than *Tethys*, or, more correctly, it seemed to have a larger but a duller disk than *Tethys*. The sky clouded completely almost before these estimations could be made and registered.

Ray Lodge, Maidenhead.
1874. November 7.

On the Duplicity of the principal Star of ν Scorpii.

By S. W. Burnham, Esq.

It will be remembered that this star was included in my Third Catalogue of the New Double Stars (*Monthly Notices*, December 1873) as being probably a very close pair, with the distance as 0".3, and position-angle about 360°. All doubt has now been removed; and in giving a brief history of this interesting star, the following from that catalogue may be quoted as giving the result of my observations in 1873:—

"I would call particular attention to ν Scorpii (No. 120), which I have very little doubt is an exceedingly close and difficult double star. I examined it several times under the most favourable circumstances, but could not get rid of an apparent elongation of the principal star in a direction nearly north and south. A power of 410 was used, and the utmost care taken in focussing. β Scorpii is an admirable object for this purpose, having about the same declination, and also a companion in the same general direction as the more distant companion to ν Scorpii. β was always seen with a perfectly round and sharply defined disk, and turning the instrument at once on ν , it did not seem possible that the slightly wedge-shaped elongation could be an illusion. Not being able to settle the question positively with my aperture, I requested Professor C. A. Young to examine it with the splendid 9.4-inch Clark refractor of the Dartmouth College Observatory, and he kindly did so. He informs me that he observed it on several occasions, generally under unfavourable